Dewey lecture

The Philosophy of Social Technology: Get on Board

I will begin with a dedication, which includes a little of the autobiography that the Dewey lectures are meant to cover, followed by two more intellectual perspectives on the field, and will finally come to the title of the talk at the end.

Dedication. I want to dedicate this lecture to two very distinguished philosophers who have died recently. Both were great friends of mine and remarkable mentors throughout my career:

Ruth Barcan Marcus, who was my doctor mother at Chicago Circle, and Patrick Suppes, who was for years my senior colleague at Stanford. They were dear friends who advised, supported, helped me from the time I first met them, throughout my career. Pat also gave me great dinners and good wine, *and*: he let me mark the students' papers in our joint seminars. Ruth gave me stern advice not to use frivolous titles for my philosophical work, like *How the Laws of Physics Lie.* These two shared a strong, visible, influential commitment to rigor that I have scrambled to emulate all my life. What else they shared: throughput the entire time I knew them, right up till when they died, I was terrified of each.

There was someone else, who was a kind of a friend, who was even more terrifying. That's Elizabeth Anscombe. To see just how terrifying Anscombe was let me tell you about an incident at one of the APA meetings a long while ago, in New Orleans, I think the year that Ruth was president. Ruth invited Kathy Parsons Addelson and me to breakfast. Kathy was one of my teachers at Chicago Circle. I was really – to use the British expression – chuffed (as in 'I was dead chuffed to have won') – I was really chuffed to be invited by Ruth, my awesome logic teacher and head of department. I was looking forward to a lovely breakfast on a charming French Quarter balcony at Ruth's hotel.

Then I learned that we were not going to Ruth's but that she was taking Kathy and me to Elizabeth Anscombe's. I tried hard to back out. But Ruth wouldn't let me. Kathy and I had to go, she insisted, as back up, to give her moral support – because she found Elizabeth so terrifying.

I talk about Anscombe because this is supposed to be in part an autobiographical lecture. And Elizabeth Anscombe played a special role for me: she was the first woman philosopher I ever saw. I was an undergraduate at the University of Pittsburgh majoring in mathematics, minoring in physics, and a dedicated philosophical groupie. There were no women on the philosophy faculty when I was there, which was not unusual then. In fact, I had no women professors at all as an undergraduate. I did have one woman teacher, in my first year of Russian. But she was a native speaker and it was made clear to us – somehow – that she was beneath the salt and that we were much better off the next year when the class was taken over by a hot-shot American male grad student.

So Elizabeth Anscombe came to Pittsburgh, brilliant philosopher famous also for raising seven children and chain-smoking cigars. Her talk was memorable, on the asymmetry of pain and pleasure. We often fail to avoid a pain, she noted, and for many reasons. But we *need* a reason for failing to avoid a pain. On the other hand, we may fail to pursue a pleasure – and for this we do not need to have any reason. For instance, she explained in a clipped British pronunciation that I cannot imitate, "Many of my friends tell me that they take great pleasure in shitting. I myself do not choose to pursue this pleasure. But I do not feel that I need an excuse."

I tell you this so you will understand why I was more than a little overwhelmed at the thought of chatting with Anscombe over breakfast in New Orleans. (Though it turns out I need not have been so terrified since Elizabeth turned out to be friendly, not fierce: she even used to sign herself in for squash courts at her college so I could have somewhere to play when I visited in Cambridge in 1982.) I also tell it to lay the groundwork for describing one of Ruth Marcus's great contributions to philosophy that I think many do not even know about: at Chicago Circle. My undergraduate mentor Jerry Schneewind wanted me to try to go to Princeton for graduate school, which he loved. But, rightly or wrongly, I felt that those prestigious East Coast grad programs encouraged a kind of combativeness that I wanted to avoid. So: Jerry sent me to Ruth's new department at Chicago Circle where I could – I think these were his words – "learn to be a woman philosopher."

Recall that at Pitt, Anscombe was my single encounter with a woman philosopher. At Circle there were, of course, guys, wonderful philosophers, some who came from our Division, like proof-theorist Bill Tait, who had come from Stanford, officially my thesis supervisor, who eased me out of logic and into philosophy of physics without ever actually voicing that I would never manage to become even a 2nd-rate logician; and some who have ended up in the Pacific division: Terry Parsons, Brian Skyrms, and Wilfrid Sellars-expert Jeff Sicha, who went to Northridge and now raises goats and runs Ridgeview Press. But the glorious thing: suddenly, there were women, 5 of them: Ruth Marcus, aesthetician Marsha Eaton, Kathryn Parsons Addelson – a model for how to weave together philosophy and left-wing politics, feminist Sandy Bartky, and Suzanne McCormick Thalberg, from whom we learned Kant. That's what happened when we got Ruth Barcan Marcus to run a Department.

Transition. So much for the dedication and introduction. For the bulk of my talk I will concentrate, first, on method in philosophy, in particular on a view about method that has dominated my work across my career, that is shared by many others – as in the Philosophy of Science in Practice Society, and that I urge be adopted more widely. Second, I will talk about the Stanford School in philosophy of science, a topic closely related to the first, and one I think particularly apt for a Pacific Division Dewey Lecture. I characterize the Stanford School by three connected themes: pluralism, particularism, and concern with practice. I mention that now so that you can watch for traces of those themes as I talk about method. The Stanford school is epitomized for me by Pat Suppes' demand: "Let's get down to the details." I couple that with Suppes' insistence on rigor.

Method. I've been discussing philosophical method with my UCSD colleagues this past term sparked by a question I asked a colloquium speaker.

> You have given a long argument with many separate premises. In an argument like that the conclusion fails if any of its premises fail. Now no step in your argument is entirely uncontroversial; each is at least a little dicey. The probability, then, that one or another, somewhere along the line, is false is significant. So why should I trust your conclusion, even if I cannot spot where the error lies?

Some of my colleagues thought this question rude: What was the speaker supposed to reply? I had not meant it to be rude. I meant to invite the speaker to thicken the argument, to bulk it out by pointing to a variety of different sources of support for the conclusion that there had not been sufficient time to present in the talk.

But it was not always the case that I intended not to be rude with remarks like this. For instance, as an undergraduate I was not appointed editor-in-chief of *The Pitt News* but passed over in favor of an inexperienced newcomer. That's because, following a run-in with one of the deans, I had written an editorial accusing him of obfuscating clear political issues with rational discourse: Giving a long argument with every appearance of soundness for what were obviously flawed conclusions, distracting us from political action into footling exploration of where the premises might have gone wrong. You can hear the angry undergraduate in this accusation. This was after all the time of the civil rights movement, the marches on Selma, Martin Luther King, and the assassination of JFK. And in philosophy of science, of Paul Feyerabend's famous *Against Method*.

So from the very start of my career as an undergraduate at Pittsburgh I have opposed

> Tall, skinny arguments that are sparse and tidy

In favor of arguments that are

> Short, stocky, and tangled.

'Stocky' is wide – the arguments cover a lot of the territory under the conclusion – and solid. I use 'tangled' to describe a rich network of interrelated arguments, each firmly attached to the ground, some with shared premises but where a great many also have a number of independent premises and, importantly, premises that come from a variety of different places outside the immediate domain in which the conclusion lives. What we do not want, I urge, are arguments that shoot up like Alice when she drinks from the little bottle.

That's what's behind the book that launched my career: *How the Laws of Physics Lie.* The laws I had in my sights there were the abstract laws of high physics theory – lofty claims indeed. What I objected to was, as

Bas van Fraassen put it, "taking them literally" as opposed to seeing them as tools, tools for the construction of more concrete, down to earth – let's get down to the details – models that could reasonably be taken as true of the local situations they represent, or at least true enough for the nonce.

One usual source of trouble for laws like these is their shear generality. They are meant to cover far more than just the phenomena that support them. So there is plenty of space for sag in between the supports. That's what I came to focus on later in the book *The Dappled World*. But in *How the Laws Lie* my concern was about the laws even at the very points that the phenomena are supposed to support them. At first sight, the high laws of physics should get a mixed verdict by my criteria. True, the conclusions are high, but the arguments for them are stocky – wide and solid – and tangled, or so they seem.

What I noted, following what I later discovered to be similar concerns about abstract science of Otto Neurath, was that the tangle of empirical results and observations that is supposed to support the laws is not actually covered by them. To get from the laws to this tangle takes a lot of additions and subtractions. And a great many of these are ad hoc. They are there just to bend the laws to fit the phenomena. So there are essential premises in the arguments for these high theory laws that have little – often no – support. That's what I meant by "they're ad hoc." In fact, I argued, this is not just the case with the very-high-theory laws but looks to be the case with almost every step up when we try to unify different empirically adequate descriptions together under a more abstract theory. There may be a thick tangle of empirical support below but there are also essential props for the next layer up that are very thin indeed, grounded if at all not in a network of empirical support but in an a priori metaphysics of unification.

Of course, by my own admission, the physics base for its high level laws is a thick tangle, so it would not matter if only a few of the connectors from the tangle to the next layer up are wobbly. But I found that for every case I investigated where theory was genuinely useful in producing models that accurately describe real-world phenomena – like the use of quantum theory to design a laser or a transistor, or to explain the behavior of a superconductor or a chemical bond in a real setting – every one of these cases was rife with ad hoc additions and subtractions, often substantial. Now the failure of the arguments for them does not show that the laws are false. Rather, I urged, it shows that it is a big leap of faith to move beyond the usefulness of the theory to count it as true, or getting on for true in the right way.

That's about physics. What about philosophy? I try to practice what I preach. And it seems that sometimes I succeed. So much so that when I was thinking I would like a job in the UK, my late husband, Stuart Hampshire, told me that he didn't think I could get a professorship at a distinguished university there because I "had one thin book and it didn't have any arguments." I think he meant it didn't have any of those tall, thin, sparse and tidy arguments that at least some British academics adored. It was, though, full of a tumbleweed of short stocky arguments of the kind I commend – or so at least I hope. (I should add parenthetically that nor was Stuart himself given to tall skinny arguments; he thought it crucial, ala Suppes, to focus on the details, as I shall explain when I turn to the particularism of the Stanford School).

Philip Kitcher, in his *Deaths in Venice*, has a different account of what I try to do, and I can't help quoting him because he puts me in such distinguished company: Wagner and Joyce. These, he says

> … do not argue…Nevertheless, they do philosophy, *real* philosophy that can lead listeners and readers to improved perspectives on a (if not the) central philosophical question. The philosophy lies in the showing. Instead of a rigorously connected sequence of clear and precise declarative sentences [I think he means a tall, skinny sequence], we are offered a rich delineation of possibilities—accompanied by a tacit injunction: Consider this.
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> This philosophical method, the method of showing, is not so far from some excellent work by professional philosophers. Some years ago…Nancy Cartwright told me rather ruefully that her husband, Stuart Hampshire…complained that she never offered arguments for her views. After some reflection, I suggested what I intended as consolation as well as diagnosis: Cartwright's typical strategy is to describe, exactly and in rich detail [-- as Suppes commends], some scientific work of a type overlooked by orthodox philosophical accounts; by considering the phenomena she portrays, her readers are expected to recognize the superiority of the precise claims about the sciences she offers as replacements for orthodoxy.[1]

I am consoled by Philip's account. But I still urge that what is going on here is argument, rigorous argument. But not the long skinny kind. Rather, a tangle of rigorous arguments, each short and stocky to ensure that the conclusions fit the precise details of the premises.

I said that this method of argument ties together work throughout my entire career. I'll give two more examples to illustrate, one from a while ago, the other recent.

In the book *The Dappled World* I again, inter alia, took on the laws of physics. This time I worried about the gaps between regions purportedly supported by our empirical findings. In brief what I urged is this. Consider your favorite equations of physics that provide a host of successful predictions. We have evidence for these in a precisely specifiable domain:

> Those situations where the effect in question is produced entirely by causes that fall under the concepts that the theory provides.

That's trivial: it is only in these situations that we know what the theory predicts. For all these equations there are huge numbers of counterexamples where the theory does not supply correct predictions. If you think that the theory nevertheless covers these cases, you must be supposing an additional premise: that there are causes at work in all these cases that do fall under the concepts of the theory, or concepts the theory may properly develop, but we poor post-lapsarians have not yet figured out what these are.

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[1] Kitcher, Philip. 2013. *Deaths in Venice: The Cases of Gustav von Aschenbach.* (New York: Columbia University Press), 23-24.

There are of course any number of defenses one can raise in support of this premise, from ones housed in the history of science to those in a priori metaphysics. But then: the arguments that insert these defenses into the support for the wide-application claims become taller and very thin in essential spots. Whether these arguments are tidy or not, the support for their premises is certainly very sparse compared with the stocky tangle that we should expect to see in support of physics claims.

I don't tell you this now to try to convince you of my conclusions about the laws of physics or about the dappled world but rather to point out the approach employed in both cases. That is: Figure out what the arguments are in their full details. Then do not embrace the conclusions if the arguments are tall and thin with sparse support for essential premises. I don't deny that such arguments can be beautiful, elegant, an intellectual triumph. Nor do I urge that we give up the intellectual challenge of constructing them in philosophy. But where security of conclusions matters, we had better stick to arguments that are short, stocky, and tangled.

My final example is from my recent work on evidence-based policy, where randomized controlled trials (RCTs) are touted as gold standard evidence for "what works." But what on earth does "what works" mean? We can, with a handful of fairly uncontroversial premises about the relations between causation and probability, show that in an ideal RCT if there is a positive difference in mean outcome between treatment and control groups, then the treatment must have produced the outcome for some individuals in the treatment group. So, the treatment worked for some members of the study population. But apparently the aim was to establish "what works"… where the expression is used as if it means "what works generally," or … "for the most part," or "it's what you should bet on in a new situation unless you have reason to the contrary." What supports that different conclusion? In almost all the literature, rigor gives out entirely here. That's what I have been spending my time recently outside philosophy trying to get people worried about.

We have a good short argument, I call it "the RCT argument,"[2] starting from positive results in the RCT, ending in the conclusion that the treatment worked for some individuals there. It is tidy, not tangled, but it is stocky… every premise well grounded. Suppose we want to export some aspect of the results from the study population to a new target population; consider, for instance, the mean difference in outcome values between the treatment and control groups, which can be seen as a measure of the causal oomph of the treatment in that population. When will it have the same oomph in a new population? Just from looking at the premises of the RCT argument I can tell you exactly when:

When

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[2] Cartwright, Nancy and Jeremy Hardie. 2012. *Evidence-Based Policy: A Practical Guide to Doing it Better*. (New York: Oxford University Press).

1. The causal principle for the new population actually has the treatment in it just in case the causal principle for the study population does.

And

2. The mean of the interactive factors for the treatment to produce the effect in the new population is the same as that mean in the study population.

Interactive factors are those necessary helping factors without which the treatment cannot act; like the presence of oxygen if striking a match is to produce a flame.

Once we've added these essential premises, the argument that starts with the result of an RCT in a study population and ends with a prediction about the causal oomph of the treatment in some target population grows *very* tall. And I have seldom seen a case where it is anything but exceedingly thin at essential places. 1. and 2. are virtually never explicitly recognized. Sometimes nothing at all is said about the move from one population to another. Sometimes we are told that it is a default position that the oomph will be the same – that it will be the same unless we see reason to the contrary. Sometimes it is supposed that the populations will be the same in the relevant respects – which we know to be described in 1. and 2.though that is not mentioned – if they are similar enough along some dimensions that we take to matter, like the US Department of Education's one warning that trials on white suburban populations do not constitute strong evidence for large inner city schools serving primarily minority students.[3]

These are tall arguments and depressingly thin at places where it matters. Unlike many of those that I wonder at in philosophy, they are not elegant. They are downright sloppy. At any rate, elegance here would be no excuse. The conclusions of these arguments are used for setting policy. They can make a huge difference to people's lives. Here we have a right to demand security in the conclusions, and in tandem, the short, stocky, tangled arguments that it takes to produce security. It is irresponsible – no: reprehensible – to parade the tall skinny arguments I see all over evidence-based policy as if they made for serious support for their conclusions. Of course often when it comes to social policy, we have to do something long before we can hope to have sufficient grounds to predict with much certainty that what we do will work. That is the human condition and that does not make it wrong to act. But it is wrong to act under the pretense that we will likely achieve what we aim for because our prediction to that effect is backed by good argument. The relevant criterion of goodness here is security and that is just what, I argue, these tall skinny arguments cannot provide.

I promised a connection between these reflections on the shapes of argument and the Stanford School. So let us turn now to that.

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[3] *Identifying and Implementing Educational Practices Supported by Rigorous Evidence: A User Friendly Guide*. 2003. US Department of Education: Coalition for Evidence-Based Policy. http://www2.ed.gov/rschstat/research/pubs/rigorousevid/rigorousevid.pdf.

The Stanford School. Here are the core members of this school who were on the faculty while I was at Stanford: Pat Suppes, Ian Hacking (to whom I was then married), Peter Galison, John Duprè, and me. There were also graduate student members of the Stanford School at the time: Hasok Chang, Jordi Cat, and Naomi Oreskes. Rasmus Winther, a later member, a student after I left Stanford, organized a conference to celebrate it recently. Here is his take on what it is:

> Since the group we call "The Stanford School" is characterized more aptly as a network and genealogy than as an ideology or phenotype, it might be worthwhile considering what's in a name. Similarly to the architectural movement of Bauhaus physically rooted in buildings in Weimar (and later Dessau and Berlin), our group is rooted in an actual school—a place—Stanford University. Analogously to the literary Bloomsbury Group, many members of our group were close friends. [NC: In fact all of the core first generation core were.] Finally, akin to the Vienna Circle, our group is very broad, bringing together creative thinkers and doers from many walks of life. This third analogy is worth exploring a bit further. Both groups were subject to diasporas. On the one hand, Rudolf Carnap took appointments at Chicago, Harvard, Princeton, and UCLA; Herbert Feigl at Iowa and Minnesota; Otto Neurath fled to Holland and Oxford. On the other hand, and independently of world events, Cartwright moved to the London School of Economics, Hacking to Toronto and the Collège de France, Dupré to Exeter, and Galison and Godfrey-Smith to Harvard; Suppes stayed. These geographic movements help spread ideas and inspirations across the world.
> ...
> We [the Stanford School] moved beyond the Logical Positivists so-called "received view," opening up vistas towards the experimental life and intervention rather than just theory and representation; towards scale models, visual models, and classifications in addition to mathematical models; and towards disunity and dappledness instead of unity and layer cakes. Indeed, the work and vision emanating from this group fruitfully balanced logic with broader discourse, form with purpose, and the rational reconstruction of theory with complex, philosophically-relevant historical and sociological detail. For instance, Cartwright's recent development of the "toolbox" view of science—represented in the conference poster, and with scientific theory as just one tool in science's toolbox—is an example of a focus on pragmatics.[4]

Apparently I coined the name, as Jordi Cat explained at the 2013 conference: "As far as I know, the term 'Stanford School' was first introduced by Nancy Cartwright for the purpose of designating and describing a group of co-workers engaged in social and intellectual interaction around a place and time, loosely determined by a strand of similarities between projects and views."[5] What views? As I outlined at the start, I single out particularism, pluralism, and attention to practice; also, consonant with its particularism and

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[4] Winter, Rasmus. Opening Comments for the Stanford School of Philosophy of Science Conference, Oct 14, 2013.

[5] Cat, Jordi. (ms.) *The Stanford School as a Historical and Philosophical Model: Poetics, Pragmatics and Politics of a Historical Project and Event.* (University of Indiana).

pluralism, is its general scepticism of "the big system" – and, I urge, the tall argument! For me the central link with my long-standing demand for stocky argument is the insistence on getting the details in, and in getting them right. The pluralism is consequent upon that. As Philip Kitcher says, in keeping with Joyce and Wagner, "the philosophy is in the showing." We did not, I pray, neglect rigor – how could we under the eagle eye of Pat Suppes? But when you want to get the details right and maintain high standards of rigor, in my experience you can't build arguments very far up. I personally delight in the details of the particular. The book I mentioned earlier, *The Dappled World*, borrows its title from the poem *Pied Beauty* by Gerard Manley Hopkins: "Glory be to God for dappled things – / For skies of couple-colour as a brinded cow; ..."

Stuart Hampshire, whom I shall soon connect with the Stanford school, had the same aesthetic views it seems. As biographer John Companiotte notes: In a review for *Encounter* magazine of Eliot's book *On Poetry and Poets* Hampshire "chastise[s] Eliot for insisting that there was ever orthodoxy of belief that added merit to verse, as opposed to various poets of different temperaments and outlooks creating unique works of art, each valuable for specific reasons."[6] Despite the aesthetic and moral appeal of the "plotted and pieced," "all things counter, original, spare, strange, fickle and freckled," I admit that life, and in particular prediction, would be far simpler and easier to manage if everything could be herded into a few intelligible categories whose members could be relied on to behave the same. So I never start a study intending to be a pluralist. But I regularly end up there… and I think that's because I care about the details. I have fairly bad eyes, so in some domains I can get things to look pretty much the same by taking off my glasses. Then everything is a bit of a blur and different faces look alike to me. Or, as in some of the examples I described in physics where you have to make a lot of substantial changes, knocking off a lot of bits, to get the empirically adequate models to fit inside the theory box. Or where, as Thomas Kuhn stressed, you get things into the box by excluding those that don't fit from the domain of the theory.

When we were at LSE together, Marco Del Seta and I discussed this particularist attitude – pay attention to the details -- in a paper titled "The Myth of Universalism – Theories of Science and Theories of Justice." [7] There we cited Eric Auerbach, one of the foremost philologists of the first half of this century, writing in defense of criticisms of his book *Mimesis*:

> My efforts for precision are directed towards single and concrete cases, while the general formulas indispensable for comparing, grouping together and defining phenomena with respect to one another were supposed to be fluid and elastic. They were supposed to adapt on a case by case basis to the capacities explicit in the single object, and must be interpreted on a case by case basis in

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[6] Companiotte, John. (With publication pending for 2016). *Philosophers Go to War: How Oxford Dons Help Win World War II.* (jcomp@mindspring.com).

[7] Cartwright, Nancy and Marco Del Seta. 1997. "The Myth of Universalism: Theories of Science and Theories of Justice." *Discussion Paper Series*, Centre for Philosophy of Natural and Social Science, London School of Economics.

> terms of the context. [Eric Auerbach, "Epilegomena zu Mimesis" [1], our translation from Italian original.]

Or probably closer to our hearts in this room, and from Oxford, which will soon play a part in my story, J.L. Austin: "It must be remembered that there is no necessity whatsoever that the various models used in creating our vocabulary… should all fit together neatly as parts into one single, total model or scheme of, for instance, the doing of actions. It is possible, indeed highly likely, that our assortment of models will include some, or many, that are overlapping, conflicting, or more generally simply *disparate."*[8]

More from Austin, in the footnote to this passage:

> This is by way of a warning in philosophy. It seems too readily assumed that if we can only discover the true meanings of each of a cluster of key terms...that we use in some particular field (as, for example, "right", "good" and the rest in morals, then it must without question transpire that each will fit into place in some single, interlocking, consistent conceptual scheme. Not only is there no reason to assume this, but all historical probability is against it. ... We may cheerfully use, and with weight, terms which are not so much head-on incompatible as simply disparate, which just don't fit in or even on. Just as we cheerfully subscribe to, or have the grace to be torn between, simply disparate ideals – why must there be a conceivable amalgam, the Good Life for Man?[9]

Oxford plays a major role in my story because I want to stress another central aspect of the Stanford School that is often overlooked, the same aspect that may be overlooked when we philosophers talk about the Vienna Circle – which was one of the shared research topics of the Stanford School. As Jordi Cat explained, the Stanford School is a group of scholars with overlapping interests, doctrines, arguments, approaches, and attitudes. Attitudes to what? Science and how to study it? There is much more to it than that. And consciously so, at least on my part. John Perry, insightful as always, once told me: Maybe you never write on morality and politics but I think you read your philosophy of science directly from your moral and political philosophy.

Particularism, pluralism, and general skepticism about any big system that can tell us how it is or how it should be, these are not philosophies peculiar to science. They make, as Otto Neurath put it, a *philosophy to serve life*. It is no puzzle that Peter Galison wrote about Aufbau and Bauhaus in Red Vienna; that Jordi Cat and Hasok Chang and I wrote about Neurath, who was head of full social planning in the short-lived

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[8] Austin, J.L. 1956. "A Plea for Excuses: The Presidential Address." *Proceedings of the Aristotelian Society* 57: 28-29.

[9] Austin, "A Plea for Excuses." fn16.

Bavarian red socialist republic at the end of WWI; that John Duprè attacked the view that what is rational to do is what maximizes your expected utility and that to call animal behaviour *rape* is a political act; that Ian Hacking is a hero of the left, seen as the spokesperson for those who are prodded, labelled, saddled with false consciousness, and 'treated' by society for their problems; and that Suppes throughout his life underlined his libertarian convictions and worked for education for the disadvantaged.

If we focus on Stanford School views as exclusively a philosophy of science we are apt to miss two central figures in the discussions—first, my late husband, the moral and political philosopher Stuart Hampshire, colleague at Stanford for 5 years and close friends with Duprè, Galison, and Suppes. Though 'Stanford School' fits, Hampshire is better known as part of a different group, a good generation older and a continent away: the Oxford pessimists, which included Isaiah Berlin and Bernard Williams, who denied the possibility of moral and political theories that can fix what is right and just and urged that we attend to what can be said and done and negotiated in the particular. We may also miss the connection with Helen Longino, now at Stanford, a friend and colleague just up the road at Mills College at the time, beginning then to develop her views that objectivity consists not in consistency with universal value-free rules but instead in serious detailed critical discussion. In negotiation… a shared theme with Hampshire on the political side and Galison on the science side. According to Longino, particularism, pluralism, and an emphasis on practice are shared Feminist perspectives on gaining and modelling knowledge. Feminist perspectives, she tells, tend towards "basic anti-reductionism," an 'emphasis on practical dimensions of enquiry," and a focus on science "in the social and material world."[10] Let's look at the Oxford pessimists. The term I think came from a review of Stuart's book *Justice is Conflict* in the *Telegraph*:

> The last half a century has seen the emergence in this country of a group of thinkers who have given a new direction, or at least a new mood, to moral and political thought. The kernel of the group is furnished by Isaiah Berlin, Stuart Hampshire and Bernard Williams but they have allies and younger followers.
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> A number of characteristics mark these philosophers out. Most fundamentally they are all staunchly anti-rationalist. Thus where most modern moral philosophers have wanted to work our moral intuitions up into a system, these philosophers doubt that there are always right answers to moral questions. There are many irreducible values, they argue, and they often conflict: each of us has to make compromises and sacrifices as best we can.

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[10] Longino, Helen. 2008. "Values, Heuristics, and the Politics of Knowledge" in *The Challenge of the Social and the Pressure of Practice.* eds. Don Howard, Janet Kourany, and Martin Carrier. (Pennsylvania: University of Pittsburgh Press), 68.

> At the same time, these philosophers are all, politically, left-of-centre; "value pluralism" is connected in their minds with liberalism or social democracy. Finally they were all educated at Oxford and have taught there for substantial parts of their careers.
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> For this reason they could be called the "Oxford pluralists." Reading Stuart Hampshire's short book, however, it occurs to me that "Oxford pessimists" might be more appropriate.[11]

Berlin too was a 'values pluralist' and opposed to 'the one big system' having been deeply affected by the twin attempts at it when he was a young man – Nazi-ism and Communism. Nor was it only the concept of liberty that he saw as bifurcated. This is from the last essay he wrote, for the *New York Review of Books* in 1998:

> I came to the conclusion that there is a plurality of ideals, as there is a plurality of cultures and of temperaments. I am not a relativist; I do not say "I like my coffee with milk and you like it without; I am in favor of kindness and you prefer concentration camps" -- each of us with his own values, which cannot be overcome or integrated. This I believe to be false. But I do believe that there is a plurality of values which men can and do seek, and that these values differ. [12]

Bernard Williams was also a values pluralist. He argued forcefully against all the great moral systems we philosophers indulge in: Kantianism, utilitarianism, virtue ethics… These do not literally make sense. Yet he did not think, as Freddy Ayer did, that moral claims can make no sense. To the contrary, Williams maintained, we can speak moral truths – we all do, using thick, Janus-faced expressions, expressions that are both "world-guided" and "action guiding": sleazy, lazy, heroic, prudish, a good sport. We can speak truths with concrete, morally-local expressions like these – until we adopt one of those nonsensical moral systems and impose the action-guidance contained in our expressions onto them from the moral system above. Then we can no longer speak moral truths. Adopting a moral *system* destroys our moral knowledge.[13]Again, as before, I do not tell you this to convince you of these conclusions – though I commend the conclusions to you – but rather to make vivid the common ground between the Stanford School and the Oxford pessimists. Stuart was a member of both. John Duprè maybe a little too? As for me, John Perry was right – as almost always. My views in philosophy of science are a mirror of my views in moral philosophy: particularism, pluralism, and attention to practice.

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11 "An Oxford Pessimist" *Telegraph*. November 1999. Accessed July 2015, http://www.telegraph.co.uk/culture/4719061/An-Oxford-pessimist.html.

12 Berlin, Isaiah. 1998. "The First and the Last" in *New York Review of Books.* Vol. XLV, Number 8.

13 Williams, Bernard. 1985. *Ethics and the Limits of Philosophy.* (Fontana Press).

Title. This brings me to the forward-looking part of this talk, and at last an explanation of its title. I want to close by urging that you join with me in creating a new field in philosophy: *Philosophy of social technology*. To explain something of my vision for the philosophy of social technology let me describe a project I'm engaged in with many others – called *K4U* – that I think of as a poster child for this new field. The long name of the project is *Knowledge for Use: Making the Most of Social Science to Build Better Policies*.

K4U aims to construct a radically new picture of how to use social science to build better social policies. The philosophical approach is broadly Popperian, for like Popper, as one of his biographers puts it, it views "science and technology as a means of understanding social problems and responding to them."[14] Following the Stanford School, it emphasises the concrete and detailed, where the real content of general philosophical concepts and claims is embodied and interrogated. How is social science knowledge to be used in policy design and deliberation – how should it be used so that policy outcomes are more effective and more reliably predictable and competing values and points of view are respected in policy choice and implementation? K4U aims to provide not just a theoretical but a practical understanding— for users: intelligible and practically helpful to those who need to estimate and balance the effectiveness, the evidence, the chances of success, the costs, the benefits, the winners and losers, and the social, moral, political, and cultural acceptability of proposed policies. To do this kind of work we need to make use of ideas, understanding, and approaches generated across standard philosophic domains: moral and political philosophy, philosophy of science, philosophy of social science, philosophy of technology (what there is of it!), studies of decision-making and deliberation, the nature of objectivity, voting theory….We have work in all of these in philosophy. The trouble is they are not working to tackle these problems, and assuredly not together and in the concrete. We need a united front. Not a unified system – which I could hardly advocate — but, as Neurath urged, unity at the point of action. That's the point of a new field, one that a young philosopher can trust will count as genuine philosophy if they put their efforts into it, a proper field that you can publish in and list on your C.V. We have nothing like this now; and we need it. Philosophy can make a huge contribution to understanding how social science is used to change society, for better or for worse, and how it can be used better. The philosophy of social technology will matter to the practices by which people's lives are affected. It can make a difference to real life.

Sadly, it is a kind of philosophy that we often undervalue. The conclusions are not lofty, the arguments are not tall – or alternatively we often say they are not "deep". So we may suppose that they are easy or involve no serious philosophy. I think we can even accuse Pierre Duhem of this mistake, despite his praise for the "ample" mind of the English as opposed to the "narrow" mind of the French. For on Duhem's

[14] Parvin, Phil. 2010. *Karl Popper*. (Continuum Books), 24.

rendering, the English mind is ample but "weak;" the French, narrow but "strong." Descartes is his paradigm of the French mind:

> What is the preface with which Descartes opens his work? A *Discours de la Methode.* What is the method of this strong but narrow mind? It consists in "conducting one's thoughts in order, by beginning with the simplest objects, easiest to know, in order to rise gradually, step by step, so to speak, to the knowledge of the more composite ones, and even presupposing an order among the objects which do not follow one another naturally."[15]

Contrast Bacon, Duhem's paradigm of the ample English mind: "Let us now open the *Novum Organum.* There is no use in looking for Bacon's method in it for there is none…. Its object is quite practical, I should go so far as to say industrial…"[16]According to Duhem, the two marks of the ample but weak mind are "one, an extraordinary facility for imagining very complicated collections of concrete facts; and two, an extreme difficulty in conceiving abstract notions and formulating abstract principles."[17] I agree that it takes a special kind of imagination, a special kind of genius – perhaps a French one! – to conceive the kinds of abstract principles we debate in philosophy, or the laws of high physics theory. But I think Duhem is not loyal to his own views here. Duhem held a view about these lofty physics principles much like mine, that they are rough templates; they organize a great deal of material under them – so long as you don't look too closely at the details and you avert your gaze from items that you might have wanted to cover in the first place but that don't fit into the box. Lofty principles do an essential job of organization for us; and they are often great tools for beginning to build accurate models. But when it is security we want in our conclusions we should look far closer to the ground. And to do this well takes great philosophical finesse. Here's what Duhem tells us about our great British ancestors:

> If the mind of Descartes seems to haunt French philosophy, the imaginative faculty of Bacon, with its taste for the concrete and practical, its ignorance and dislike of abstraction and deduction, seems to have passed into the life-blood of English philosophy. One by one, Locke, Hume, Bentham, and the two Mills. All these thinkers proceed not so much by a consecutive line of reasoning as by piling up of examples. Instead of linking up syllogism, they accumulate facts.[18]

What I want to underline is that whatever they did, we know it took Locke, Hume, Bentham and the two Mills, gigantic philosophical skills to do it.

Consider: What did Stuart do in the war? He looked at details of huge masses of information coming from a vast variety of sources that were listening in on and eyeing Germany – to make sense of what it might

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15 Duhem, Pierre Maurice Marie. 1954. *The Aim and Structure of Physical Theory.* (Princeton, New Jersey: Princeton University Press), 65.

16 Ibid, 66.

17 Ibid, 64.

18 Ibid, 67.

amount to. J.L. Austin too. He "prepared daily reports on the German defense forces in France that the allies would face in the Normandy invasion for D-Day in June 1944."[19] John Companiotte reports that it was said of Austin that "he more than anybody was responsible for the life-saving accuracy of the D-Day Intelligence."[20] Stuart connected this directly with Austin's philosophy: "I think that it is certain…that his natural love of concrete and detailed investigations, and of discoveries that gradually emerge from careful accumulations of fact, had already during the war led to fixed intentions in philosophy."[21]It was war efforts like these that gave Otto Neurath his confidence in the possibility of social planning. But war efforts provide just peculiarly striking examples. Willy-nilly, we are engaged in social technology at all levels of society all the time. And, as with almost all human endeavors, in my opinion, philosophy can help us do it better.

Which brings me to my very brief concluding sentences for this Dewey lecture. In *The Origins of Pragmatism*, AJ Ayer tells us, "One of the main features of pragmatism, which comes out … in Dewey… is that it is a dynamic philosophy. … the pragmatists put themselves in the position of an enquirer adapting himself to and helping to modify a changing world."[22] Recall, I see the Stanford School as a "philosophy that serves life." It is not embarrassed to subscribe to the Marxist view that philosophy is not just to interpret the world; "the point is to change it." I urge then, in closing, that we embrace this duty: Let us embark on the philosophy of technology and undertake to change the world.

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19 Companiotte, John. *Philosophers Go to War.* (ms.).

20 Ibid.

21 Hampshire, Stuart. 1960. "Austin, JL-In Memorium" in *Proceedings of the Aristotelian Society* 60, quoted in Companiotte, John. *Philosophers Go to War*. (ms.), 20.

22 Ayer, A.J. 1968. *The Origins of Pragmatism: Studies in the Philosophy of Charles Sanders Peirce and William James.* (London: MacMillan Press), 5-6.